many were those of long-legged wading birds. But the most interesting are those of the Mammoth and the problematical so-called human tracks. About the Mammoth tracks there can be no doubt. Some of these were uncovered by blasting in my presence; round basin-shaped impressions, 5 inches deep and 22 inches across, and occurring in regular alternating series, the hind-foot tracking almost perfectly with the fore-foot. The nature of the so-called human tracks, however, is far more doubtful. These occur in several regular alternating series of 15-20. In size they are 18–20 inches long, and 8 inches wide. In shape they are many of them far more curved than the human track, especially in soft mud. The stride is 2½ to 3 feet, and even more. The outward turn of the track is in many cases greater than in human tracks, especially in soft mud. But the most remarkable thing about them on the human theory is the straddle, *i.e.* the distance between the right and left series. This I found to be 18 and even 19 inches, which was fully as great as that of the mammoth tracks. This is probably the greatest objection to the human theory. On the other hand, the great objection to the quadrupedal theory is the apparent singleness of the tracks, and the absence of claw-marks. But it must be remembered that the tracks are deep, and the outlines somewhat obscure, and also that the mammoth tracks, on account of tracking of hind with fore-foot, are in most cases, though not always, single.

After careful examination for several days, the conclusion I came to was that the tracks were probably made by a large plantigrade quadruped, most likely a gigantic ground-sloth, such as the Mylodon, which is found in the Quaternary, or the Morothenium, which is found in the upper Pliocene of Nevada. The apparent singleness, the singular shape, and the large outward turn of the tracks I attribute to the imperfect tracking of hind and fore-foot on the same side, while the absence of claw-marks was the result of the clogging of the feet with mud.

This view seems to me most probable,[1] but many who have seen the tracks think them human, and I freely admit that there is abundant room for honest difference of opinion. On any theory the tracks are well worthy of scientific attention.

Berkeley, California, May 12 JOSEPH LE CONTE

Cloudiness of Aquarium

CAN you tell me the reason why the water in my fresh water aquarium will not remain clear, but becomes cloudy throughout in a few days after filling.

The aquarium in question holds about twelve gallons of water. It stands in a window facing north. I have in the water two or three water-plants, among them a water-aloe. At the bottom are small gravel stones, which have been thoroughly washed before using. Floating on the surface for the benefit of a few newts is a piece of virgin cork, on which is placed some carpet moss. I had a dozen minnows and four newts to begin with, but nine of the minnows and two of the newts have died, manifestly from the fouling of the water.

The framework of the aquarium is iron, with a slate floor. The glass sides are fixed with red lead. There is a copper tube for overflow purposes, which was inserted when a fountain was used in the centre. This has now been removed and the water is stagnant.

It is now some years since I have kept an aquarium, and I cannot divine the reason for the above-mentioned cloudiness of the water. I shall be much obliged if you or some of your correspondents will help me. X.

May 9

So far as I can judge from "X's" description, the cloudiness of the water in his aquarium is due to the abnormal development of some unicellular algal (Palmellaceæ) or to the prolific spore-production within it of one of the filamentous forms (Confervaceæ). This may be obviated by screening the back of the tank from the access of light. Possibly "X" may find on examination that the cistern whence he obtains his supply has been left uncovered, and that the intruding algal has established itself and entered upon the reproductive process in that position. In that case he should either isolate the water he requires in a dark place for a week or so, when the spores will die, or obtain his supply from a purer source. An investigation with a high power of the microscope of the turbid water complained of will speedily determine whether the explanation here suggested is the correct one. By way of illustration, I may mention that the water of the ornamental pond in the centre of the Horticultural Gardens, supplied clear and bright shortly before the opening of the Fisheries Exhibition, had assumed within a few days and still retains the colour and consistency of green-pea soup through the rapid development, under the action of light, of a unicellular cryptogam in the manner above described. W. SAVILLE KENT

[1] Views similar to my own have recently been expressed by Prof. Marsh and by G. K. Gilbert.

Singing, Speaking, and Stammering

REFERRING to the letters in NATURE (vol. xxvii. p. 580) on my classification of vowel sounds, allow me to explain:—

The classification given in the "Principles of Elocution" (4th ed., 1878) was retained from the earlier editions of that work, because of the difficulty, or impossibility, of exhibiting the complete vowel system of visible speech without V.S. symbols. For the purposes of the book on Elocution, the latter were not required; but a *note* (on p. 36) immediately preceding the "General Vowel Scheme" explains the basis of the complete classification developed in visible speech.

As you have given an abstract of my classification, quoted by Dr. Stone from "Principles of Elocution," I shall be glad if you will show your readers the following abstract of the visible speech classification:—

Classification of Vowels in Visible Speech

Nine Lingual positions yield 9 Primary vowels ... ...
Each Primary vowel yields a "Wide" variety by faucal expansion = 9 Wide vowels ... ... } = 18 Lingual vowels.
Each Lingual vowel yields a "Round" variety by labial contraction ... ... } = 18 Labio-lingual vowels.
} = 36 Normal vowels.

Each Normal vowel yields a possible variety by *higher*, *lower*, *broader*, or *narrower* formation = 36 + 144 = a total of 180 vowels.

The mutual relations of the different sounds may be exhibited in this way:—

LINGUAL.

	Primary.			Wide.		
	Back.	Mixed.	Front.	Back.	Mixed.	Front.
High	7	4	1	7	4	1
Mid	8	5	2	8	5	2
Low	9	6	3	9	6	3

LABIO-LINGUAL.

	Primary.			Wide.		
	Back.	Mixed.	Front.	Back.	Mixed.	Front.
High	7	4	1	7	4	1
Mid	8	5	2	8	5	2
Low	9	6	3	9	6	3

In this arrangement, each No. 1, No. 2, No. 3, &c., in the four sets is formed from one and the same lingual position. These relations are plainly exhibited in the symbols of visible speech. They cannot be shown by ordinary letters, but the use of *numbers*, as above, may make the arrangement clear to those who are not acquainted with visible speech.

Washington, D.C., May 12 ALEX. MELVILLE BELL

On the Cold in March, and Absence of Sunspots

I WAS travelling when Dr. Woeikof's letter appeared in NATURE (vol. xxviii. p. 53), and could not sooner reply to his criticisms on my communication (vol. xxvii. p. 551), "Unprecedented Cold in the Riviera—Absence of Sunspots." Let me first remark that I do not go so far as to "ascribe (as Dr. Woeikof says that I do) the great cold of March, 1883, at the

Riviera, to the absence of sunspots." My observations prove only the *coincidence* of a sudden and unprecedented visitation of cold, with an absence of sunspots (the more remarkable as occurring during a *maximum* sunspot period); and the further *coincidence* of a progressive rise in temperature with the return of the sunspots; but I add, "These observations are too few and too imperfect to warrant any decided conclusions; but they add to those already made in evidence of the connection between the absence of sunspots and the diminution of terrestrial heat; and I trust they may be followed by further and more exact investigations, to determine the influence of our great luminary on the weather and climate of the world."

It does not appear to me that Dr. Woeikof has succeeded in establishing a parallel between Cannes and Suchum-Kale on the Black Sea; which, however sheltered locally, must, far more than Cannes, be liable to chilling influences in the cold winds from the lofty mountains and vast elevated steppes to the north, extending even to the Arctic regions. Therefore the fall of 31° below average in March, 1874, might not be extraordinary, even in a *year* with a considerable number of sunspots. It is not stated that the spots continued in this particular month.

The case of Cannes may be thus stated: With a climate usually so mild in winter that frost and snow are of rare occurrence; and this winter, with slight frost only three times before February, and none at all in that month, the average minimum being 44°,—on March 7 minimum fell to 36°, with a heavy fall of snow; and on the 8th, 10th, 11th, and 12th, the minimum fell further to 27°·7, 27°, 24°·1, 25°·7. The sunspots, which had been observed by my friend, Mr. Campbell, of Islay, to be large and active until February 26, suddenly disappeared, and on February 28 and March 3 I found no spots; on the 10th and 11th only one or two small spots. On the 12th they began to appear in numbers, with a large oval facula. From that day they continued to increase, and the temperature gradually rose to the ordinary average.

I will not occupy space with further arguments, but I will merely state some more facts with regard to the extraordinary intensity and universality of this invasion of cold, and my further observations of the sunspots. At my villa at Cannes, which is favourably placed in position and shelter, the register did not fall so low as in other parts. At Dr. Frank's villa, Grand Bois, more open to the north (thermometer in louvred box, a metre above ground), the minima were: March 7, 27°; 9, 25°·2; 10, 21°; 11, 21°; 12, 20°; 13, 25°. At Villa Beaulieu, more sheltered (therm. also in louvred box), minima were: March 7, 29°; 9, 27°; 10, 25°; 11, 26°; 12, 28°. Dr. de Valcourt's minima are somewhat higher; but he adds this note: "La période de froid du 7 au 14 Mars, 1883, a été très remarquable; elle est unique, depuis que les observations regulières ont été recueillis à Cannes." Where instrumental records are wanting, we refer to the report of the "oldest inhabitants," and learn that there has not been a cold so severe or destructive to oranges and olives since the year 1820.

Extraordinary and intense as was this invasion of cold, it might have been supposed due to local or regional causes only, had it been confined to Cannes and its neighbourhood. In my former paper I stated that I was not informed how far the cold had extended to other countries and latitudes. We still need further exact information on this point, but what has already reached us goes far to prove that the cold was universal, and not limited to a region. In England, Mr. Thomas Plant writes to the *Times* from Moseley, Birmingham:—"After one of the mildest winters registered in the Midland Counties, the month of March, which is generally expected to be the beginning of spring, has been colder this year than any corresponding month for 38 years." "When we consider the power of the sun in March, as compared with December, January, and February, then we can realise some idea of the prolonged and most abnormal cold of the month now ended." By private information I learn that at the same time, in Stockholm, Centigrade's thermometer fell 13°, and at St. Petersburg 18°, below freezing. Unusually intense cold in March is also reported from Canada. In the south we hear of snow and frost in South Italy, Sicily, Algeria, Egypt, and even Nubia. Later still there have been reports of snow on the mountains of Madeira and California, where it had never been seen before.

Since March 19, the date of my former letter, I have been able to make sketches of the sun's position on 49 days.[1] Of these observations the following summary may suffice. In number the spots varied from 3 to 18; the larger showing, more or less, holes or clefts of central umbra, with fringe of penumbra. Faculæ, or clouds of whiteness, were often seen around the larger spots. The spots varied in number and form from day to day; and although the same large spots and even groups could be traced for several successive days, they never retained the same aspect during the whole period of the sun's semi-rotation. On April 17 the spots were at their maximum; in number 18, in three groups. During this period, from March 19 to April 19—thirty-one days—the mean minimum temperature was 46°·2, mean maximum 57°·9.

From April 20 to May 7 there was considerable diminution of the spots; numbers not exceeding 8; and on May 7 there was only one large spot, with surrounding facula. The mean temperature of these seventeen days was—minimum 49°·8, maximum 60.

From May 8 to 16 spots were few, from 2 to 8; but two of them were very large, with umbra and penumbra and sometimes adjoining faculæ. The mean temperature of these nine days was—minimum 52°·7, maximum 63°·8.

Here my observations terminate, as I left Cannes on the 16th, and have no means of observing in London, even if the atmosphere permitted. But I conclude by strongly commending the attentive study of the sun not only to astronomers and physicists, but also to practical meteorologists, as an interesting and not difficult addition to their work of observation, and one likely to supply information concerning the most important factor in the problems of weather and climate. C. J. B. WILLIAMS

47, Upper Brook Street, May 25

The Soaring of Birds

MY thanks are due to Mr. R. Courtenay for the notice he has taken (NATURE, vol. xxviii. p. 28) of my letter on the Soaring of Birds (vol. xxvii. p. 592). It is a great satisfaction to me to find my general conclusion supported by his observations. As to the possibility of a soaring bird utilising a downward current of air, I stand corrected. There is no difficulty in agreeing with Mr. Courtenay that the bird, finding itself in a downward current "will descend swiftly o as to acquire the necessary impetus for a rapid escape;"—that is to say, it will seek to make the best of a bad bargain. But it is not so easy to see that the bird, in a current approaching the perpendicular, will "acquire an impetus much more than compensating for the slight loss of elevation;"—that is, will actually make a profit out of a seemingly adverse condition.

This paradox, however, becomes more acceptable by the aid of an illustration:—A marble held lightly just within the rim of a hemispherical bowl, if let drop, will barely reach the opposite rim, but, if struck sharply downward, will run up the opposite side and leap up above the opposite rim. In like manner a bird, struck by a downward current as by a hammer-stroke, may speedily acquire a downward velocity greater than that due (under gravity) to the height through which it has descended; and may therefore rise, if it can escape from the downward current into a horizontal (or *a fortiori* into an upward) current, to a greater height than if it had fallen from the same starting-point through still or horizontally-moving air.

I am very much obliged to Mr. Courtenay for pointing out this interesting result. It gives completeness to the theorem, which now stands thus: that any alternations in the strength or direction of air-currents can be so utilised by birds as to enable them to soar. HUBERT AIRY

Woodbridge, May 25

The Zodiacal Light

THE phenomenon to which your correspondents allude, under the head of zodiacal light, was seen by me in the month of April, 1852. At the time I wrote a letter to the *Times*, in which I suggested it might be caused by the reflection of the sunlight at the surface of two masses of air of different densities, however irregular the bounding surface might be, in the same manner as the line of light seen reflected between the observer

[1] I use only a modest achromatic of 32 inches focus, and 2¼ inches aperture, which, projecting the solar image on a white card, exhibits the spots with umbra and penumbra, and the faculæ, sufficiently for this purpose. Of course a more powerful instrument would show a great deal more, both in number and in construction of the spots. For instance, on April 17, when I made out 18 spots, Mr. Campbell's solar image exhibited 104, with a marvellous variety in the larger spots, and in the dome-like expansion of the adjoining faculæ. But these details, so deeply interesting in heliography, are not wanted for meteorological purposes.